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in the discussions which followed its presentation to the joint committee.

IN A RECENT number of the *Indian gazette*, Dr. Klein, who, with Dr. Gibbes, is now in India investigating the cholera, attempts to throw fresh discredit upon the theory of the specific nature of the comma bacillus of cholera. The grounds for his objections are these. He examined three houses in Calcutta where there had been a severe outbreak of cholera in November. He found the water-supply of all of them good. *Per contra*, at some distance from these houses, and never (?) used by their occupants, were three tanks of water which were swarming with the comma bacilli. The natives in the immediate neighborhood of these tanks used the water freely, and yet were practically free from the disease. Therefore Dr. Klein concludes against the specific nature of the comma bacillus. If this style of *post hoc ergo propter hoc* reasoning is what we are to expect from the English commission, confidence in their conclusions will not be readily given. Koch's position is simply that the cholera bacillus is a necessary condition to the occurrence of cholera, and this latest discovery of Dr. Klein proves nothing against it. It merely seems to show, what has already been granted, that the comma bacillus may be present without the occurrence of cholera. Circumstances favoring its development are, of course, necessary; and a receptive condition of the system must be established in order to its growth,—a fact which is true of all forms of bacteria, so far as they have been observed in relation to pathogenesis.

LETTERS TO THE EDITOR.

** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.

Coal in the Chico group of California.

THE California geological survey reached the conclusion stated by Professor Whitney in the preface to the second volume on the paleontology of the state, p. xiii., that the Tejon group is the only coal-producing formation in California. In the Proceedings of the California academy of sciences, Mr. J. G. Cooper has recently published a number of notes on the coals of the state. After remarking (vol. v. p. 385) that the Vancouver coal, and others in that region, are undoubtedly of cretaceous age, he states

that "there is still some doubt as to those of California, which may be partly or entirely above the cretaceous strata."

Last summer, while engaged in the geological survey of the Cascade Range, a number of fossils were collected from the coal-bearing strata in northern California, eight miles north-east of Yreka, on the road to Linkville, Ore., and south of the cove at the Great Bend of Pit River, where considerable coal has been found. The fossils have been examined by Dr. C. A. White, who reports that they belong to the Chico group, and thus removes the doubt that some of the coal in northern California properly belongs to the cretaceous.

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Man in the stone age.

In a communication to *Science* (v. 3) Dr. Brinton charges me with having forgotten what I read in de Mortillet's 'Le préhistorique.' I am at a loss just how to characterize his quotations from that work, which, like

"The adventure of the Bear and Fiddle,
[Begin] but break off in the middle."

De Mortillet wrote (p. 248), "L'accumulation de caractères simiens dans la race de Néanderthal montre clairement que l'homme primitif se rattache aux singes. S'il ne se relie pas directement aux anthropoïdes actuels, c'est qu'il manque entre eux et lui des échelons. Certainement il descend d'une forme ou d'un type intermédiaire. *Nous nous retrouvons donc en présence de l'anthropopithéque, dont j'ai démontré l'existence* (p. 102). Il suffit d'ouvrir les yeux et de regarder pour le voir! Les anthropopithéques se sont montrés, se sont développés et se sont éteints pendant le tertiaire. L'homme a apparu au commencement du quaternaire. Cet homme primitif constitue la race de Néanderthal." Of this Dr. Brinton has chosen to quote only what I have put in Italics. He quotes de Mortillet as saying (p. 339) that the epoch of Moustier 'was characterized by the race of anthropopitheci.' What he actually says is, "L'homme de cette époque devait en majeure partie appartenir à la race de Néanderthal." Again: he says for the epoch of Solutré, de Mortillet "leaves the question open, denying that any traces of man or anthropoid have been discovered (p. 392)." His real language is, "Il résulte de tout ce qui précède que nous n'avons aucun document ostéologique sur l'homme solutréen."

I cannot pretend to be so well informed as Dr. Brinton upon 'the language, religion, and social compacts' of paleolithic man, but I do claim to know something about his *works*; and it is not 'word-splitting' to insist that the magnificent lance-heads of Volgu, in the museum of Chalons-sur-Saône, are quite as much the work of *man, properly so called*, as any 'stemmed scrapers'; nevertheless these belong to the epoch of Solutré.

I am well aware, that, in 1881, de Mortillet chose to substitute the term *chelléen* for *acheuléenne*, which he had suggested nine years previously. But the phrase 'axe of the St. Acheul type,' for the implement peculiar to that epoch, has become too firmly fixed in the nomenclature of prehistoric science ever to be misunderstood; except, possibly, by one who could say that Robenhausen belongs to the 'first epoch of the appearance of man on the globe,' disregarding all the marvellous artistic works of the cave-dwellers of Aquitaine, who belong to the preceding epoch of La Madelaine.

HENRY W. HAYNES.

Boston, Jan. 5.